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ABSTRACT

Updating a 1977 review, this review of the research concerning parents' effects on their children's development asks what progress has been made in understanding such effects during the last decade. After presenting findings of the earlier review, discussion focuses on five areas of research: temperament, attachment, cognitive development, language acquisition, and socialization. It is concluded that increased knowledge of parents' effects on children's development has revealed how much remains unknown. Progress has been made over the past decade, but the progress has been in problem finding rather than in problem solving, divergent rather than convergent. Researchers should focus on the limits of parents' effects on their children's development and conduct systematic experiments. Parent training programs can be both the medium and the message in this area of research. A 26-page reference list is appended. (RH)

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# PARENTS' EFFECTS ON CHILDREN'S DEVELOPMENT: A DECADE OF PROGRESS?

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Parents' Effects on Children's Development:

A Decade of Progress?


## Abstract

Ten years ago, I reviewed the research concerning parents' effects on their children's development and came up with a number of generalizations about our state of knowledge--and ignorance--in this area. I also made a number of criticisms and suggestions for future research. Now, a decade later, it seems appropriate to take stock of our progress: What do we know now about parents' effects that we did not then? In this paper, I review current research results and trends, focusing on five active areas of research--temperament, attachment, cognitive development, language acquisition, and socialization--and again came up with a description of our knowledge and ignorance and some criticisms and suggestions for future research. The central suggestion that comes out of both reviews is that we need to exploit more fully the under utilized paradigm of parent training in order to both understand and control the processes involved in parents' influence on their children's development.

In 1977, I was asked by the editor of the Review of Research in Education to write a review of the research concerning parents' effects on their children's social and intellectual development. It is now a decade later--and time to take stock. Have we made significant progress in our quest to learn about parents' effects? Are we moving in the right direction? Where should we go from here? In the present paper I examine the state of our knowledge--then and now--and attempt to answer these questions.

In my review 10 years ago (Clarke-Stewart, 1979), I first discussed research on animals that demonstrated clear links between development and sensory deprivation and enrichment, somewhat less clear links between development and social deprivation and separation, and questionable links between development and either sensory or social experience during "critical periods" (see also Rutter, 1974; Thompson & Grusec, 1970). This research, although interesting and valuable in its own right, had limited generalizability to the subtle patterns of child rearing and development found in human families.

I next reviewed research on so-called "natural experiments" with human subjects. This research included the observations of wan children growing up in understaffed orphanages and other residential institutions (e.g., Ainsworth, 1962; Goldfarb, 1945; Rutter, 1974; Spitz & Wolf, 1946), which also demonstrated the effects of severe sensory and social deprivation on development. These observations, like the animal studies, were limited in their generalizability and unlikely to apply to children in the normal range of families. In addition, the findings of retarded development in institutionalized children were difficult to interpret because of confounding factors of heredity, health, and nutrition in the children's backgrounds. Research on other "natural experiments" that I reviewed included studies of

institutionalized children who were later adopted (e.g., Dennis, 1973). These studies demonstrated the substantial reversibility of the effects of early deprivation--depending upon the length of deprivation and the intensity of subsequent educational efforts and support--but they also were limited in their generalizability and confounded by selectivity factors. Yet another kind of natural experiment was provided by the unfortunate children I labelled "lost and found"--children like Isabelle, who was raised by her deaf mute mother, and the twin boys in Czechoslovakia who were banished to the basement by their wicked stepmother (Clarke & Clarke, 1976) or, perhaps best known, the girl dubbed Genie, who was tied to her potty chair or caged in her crib and growled at by father until her discovery and rescue at age 13 (Curtiss, 1977). Case studies of these children also showed the effects of severe deprivation and subsequent substantial recovery as a result of attention and education. But they were even more limited than the studies of institutionalized and adopted children in their generalizability and interpretability.

The third kind of research I reviewed was studies of group differences in parents' and children's behavior. This research included studies of parents and children in different cultural groups--research which, at the time, in the words of one cross-cultural developmental psychologist was too often "the study of individual differences in a nice place without a guiding theory, hypothesis, or rationale" (Tulkin, 1977, p. 568). This kind of research was potentially useful for demonstrating "universals" in child development, child rearing, and parent-child relations, for increasing the range of variation and, therefore, the possibility of obtaining significant correlations between parents' behavior and children's development, and for shocking American researchers into an awareness of their own unexplored biases and assumptions. But these possibilities had so far been largely unrealized.

Research on differences between groups of parents and children also included studies comparing parents and children from different social classes. Researchers had documented the fact that, compared to children from middle-class families, children from poor families were deficient in their performance on intelligence tests and their achievement in school, and that their parents were deficient (or at least different from middle class parents) in distinctive and responsive verbalization, teaching strategies, and discipline (e.g., Beckwith, 1972; Cazden, 1966; Kagan, 1968; Tulkin & Kagan, 1972). The popular assumption was that the differences between the parents were causing the differences between the children. There were, however, methodological problems with these studies. Researchers had often not separated social class from race or family structure in their research designs, and they had frequently based their conclusions on assessments that discriminated against lower-class families (for example, tests in university laboratories). Most important, researchers studying differences between social classes had not usually investigated the processes of influence in the families they tested, and, consequently, they had not established that the differences they had observed between the groups of children were the result of differences between their parents.

Another kind of research on group differences that I reviewed was research on maternal employment. This research had the same problems as the research on social class. The research showed that, in general, boys under 6 years of age whose mothers were employed full time were likely to have problems in social and emotional development, whereas girls over 6 years of age whose mothers worked were likely to have enhanced academic achievement and aspirations (see Hoffman, 1974). But, like the research on social class, this research did not elucidate the causal processes underlying the observed

differences between the groups of children. Researchers were beginning to explore these processes by assessing mothers' role satisfaction as well as their work status (e.g., Harrell & Ridley, 1975; Yarrow, Scott, De Leeuw, & Heinig, 1962), but they had not examined the processes underlying the observed differences between boys and girls. Was the reason that having a working mother is good for girls that the working mother provides a model of feminine achievement, that she encourages her daughter's achievement or pushes her toward independence, or that the father is more involved with the child? Was the reason that having a working mother is bad for boys that boys are more vulnerable to stress, that they get less of their working mother's attention, or that the attention they do receive is more likely to be negative. The answers were unclear. What was clear was that simply comparing children and parents from different groups--based on culture, class, or employment status--was only one step in tracking down the effects of parents' behavior on children's development.

So I turned in my review to those studies in which researchers had actually observed the behavior of individual parents and correlated it with their children's development. This method of study had become quite popular in the late 1960s and 1970s and a substantial number of correlational studies were available (e.g., Ainsworth & Bell, 1969; Baumrind & Black, 1967; Beckwith, Cohen, Kopp, Parmelee, & Marcy, 1976; Clarke-Stewart, 1973; Elardo, Bradley, & Caldwell, 1977; McCall, Appelbaum, & Hogarty, 1973; Nelson, 1973; Schaffer & Emerson, 1964; Yarrow, Rubenstein, & Pedersen, 1975). The most consistent generalizations to emerge from these studies were the following:

-- Parents' caretaking practices (such as the method of feeding or toilet training or the time taken up with these practices) are not related to children's development.

-- Mothers' availability (such as the amount of time the mother spends in the same room as the child) is not related to children's development.

-- Mothers' more frequent expression of affection is related to children's social-emotional development, but it is not consistently related to their intellectual development.

-- Mothers' stimulation (the amount of rocking or jiggling the infant, talking to or playing with the toddler, and stimulating the child with objects that are age-appropriate and varied) is related to children's intellectual development, and the amount and quality of the mother's verbalization to the child (number, variety, complexity, and responsiveness of utterances) is related to the development of language comprehension and vocabulary.

-- Mothers' responsiveness to infants' distress and toddlers' social signals is related to the frequency of infants' crying and vocalizing and to children's exploration, cooperation, sociability, compliance, communication, and cognitive development. There were hints also in the research that this link between parents' and children's behavior is not just the result of simple reinforcement of the child's behavior, but the result of the child's developing a generalized expectancy of being responded to, which leads him or her to approach and explore the environment. For example, mothers' responsiveness to infants' social expressions was more highly related to the infant's I.Q. than to the subsequent frequency of the infant's social expressions (Clarke-Stewart, 1973).

-- Parents' discipline and control of their children is related to the children's development. Both parents who exert too much control--being restrictive with their infants and directive with their children--and

parents who exert too little control--being excessively permissive and lenient--have children with less adequate development. Research pointed to the benefit of a happy medium--an "authoritative" disciplinary style (Baumrind, 1967).

Of course these correlational studies, too, had methodological flaws. Observations of parents and children were often made in laboratory settings rather than at home, and even at home, visits tended to be few, brief, and limited to certain convenient hours. Samples were usually small and not necessarily representative of the normal population. Although researchers had gone beyond simple frequency counts in their observations, they had seldom operationalized and assessed complex constructs such as maternal responsiveness or sensitivity. The major problem, however, was that most of the researchers had interpreted their correlation coefficients as evidence that parents' behavior was responsible for the differences in children's development. This inference was clearly unwarranted. But correlation coefficients had not yet been replaced by alternative designs (like repeated measures) and analyses (like causal model analyses) that might provide ways of ruling out implausible causal hypotheses. Bell's (1968) article highlighting the effects of children on parents' behavior had inspired researchers to document children's contributions to parent-child interactions and relationships. Researchers had performed microanalyses of mother-infant cycles (e.g., Brazelton, Koslowski, & Main, 1974; Jaffe, Stern, & Peery, 1973), studied how maternal bonds were affected by early contact with newborns (e.g., Klaus & Kennell, 1976) and examined the effects on parents' behavior of individual differences among infants in responsiveness (Osofsky, 1976), birth weight (Brown, Bakeman, Synder, Frederickson, Morgan, & Hepler, 1975), sex, and smiling (Clarke-Stewart, 1973). Nevertheless, this research had not

really helped crack the causal conundrum of parents' effects on children's long-term development. There were only three studies I knew of that used causal analyses over a longer time frame (Clarke-Stewart, 1973; Kessen, Fein, Clarke-Stewart, & Starr, 1975; Rogosa, Webb, & Radin, 1978). The results of these studies did support the hypothesis that the direction of influence was from maternal stimulation and responsiveness to children's intellectual development, but they could only be considered suggestive. (This was particularly so because researchers in the first two studies had used cross-lagged panel correlational analyses, which researchers in the third study claimed was inappropriate.)

Another problem with observational studies of parents' and children's behavior was that researchers had drawn mistaken conclusions about the relative importance of early and later experience on the basis of the correlations obtained. The claim, for example, that there is a critical period for children's intellectual development between 12 and 24 months (White, 1975) was based on the finding that correlations between parents' behavior and children's intelligence before 12 months were not statistically significant whereas past that age they were, and that correlations between parents' behavior and children intelligence (or between successive I.Q. tests) after 24 months were consistent across time. This conclusion failed to take into account changes in the content of I.Q. tests before and after 12 months and the contribution of stable environments to stable I.Q. scores (i.e., finding stable I.Q. scores and stable parent-child correlations does not mean that the later environment is not important, but just that its effects are not different from earlier effects).

A further problem with the observational studies of parents' and children's behavior was that they were basically univariate. Most researchers

had calculated correlations between a few pairs of isolated variables and concluded that, if significant, the child-rearing variables they had selected were the critical factors influencing child development, when, in fact, the critical influence could have been some other, unmeasured variable that was correlated with the observed ones. The few multivariate studies that were available showed that all good things for parents and children go together, and suggested that it is risky to assume that you have found a magic bullet until the arsenal has been thoroughly searched. Making the situation even more complicated, researchers who had assessed multiple variables and compared their relative predictability of children's development had not gotten consistent results. For example, McCall et al. (1973) had found that the only "essential" part of the parental cluster they assessed was the mother's deliberate stimulation of the child's activities, whereas Clarke-Stewart (1973) had found that mothers who were low on level of stimulation but highly responsive had children with higher I.Q. scores than mothers who were highly stimulating but not very responsive. Clearly, continued multivariate efforts were needed. Indications also were beginning to appear that suggested that processes of parental influence might differ depending upon the child's age, sex (e.g., Baumrind, 1977), and race (e.g., Clarke-Stewart, 1973). Multivariate designs were necessary for revealing differences in parent effects for different subgroups of children.

Another aspect of the multivariate family scene--fathers--had just begun to be incorporated into correlational studies. In the 1970s a number of studies had been undertaken to show that fathers are as good as mothers at caretaking and better at playing (e.g., Lamb, 1976; Parke & O'Leary, 1976; Yogman, 1977). But these studies kept mother and father separate. There were only a few "triadic" studies showing interrelations between mothers' and

fathers' behavior as correlated with the child's behavior, and these were based on small samples (e.g., Clarke-Stewart 1978, n=14), and so needed replication and expansion.

In my review I next discussed research using experimental designs that were relevant for understanding parents' effects on children's development. There were several studies in which researchers had intervened experimentally in the lives of children growing up in institutions (e.g., by placing mobiles over their cribs; White, 1966). These studies demonstrated the effectiveness of supplementary care and stimulation on children's development, at least against a background of extreme deprivation. The bulk of experimental studies, however, involved using an experimenter as a parent substitute--an experimenter who stimulated, reinforced, punished, or modeled for the child in the laboratory (see Bandura, 1977; Bryan, 1975; Gewirtz, 1969; Risley & Baer, 1973). These studies demonstrated marked and immediate short-term effects, which sometimes transferred to other situations. The effects were especially likely if the experimenter had parent-like characteristics, such as power, prestige, competence, and nurturance. Few researchers had used children's own parents as the experimental reinforcers or models, but when they did (e.g., Waxler & Radke Yarrow, 1975) they discovered that the effect of the parent's behavior depended upon the pair's prior history of interaction. Because children's prior histories had generally been ignored in laboratory studies, and also because laboratory situations were highly simplified and artificial, these experimental studies did not necessarily reflect the processes of parents' influence actually occurring in families.

The best test of causal links between parents' behavior and children's development, I suggested in my review, is parent training. At the time I was writing the review, it seemed as if we were in the midst of a national

movement toward training parents. Inspired by the claims of the Commissioner of Education, Terrell H. Bell, that "Parent education is the key to more effective education" (1976) and "Every child has a right to a trained parent" (1975), spurred on by "Head Start Has Failed" headlines, and facilitated by the availability of federal funds for research and demonstration projects, social reformers and researchers had turned to family intervention as a way of effecting social change. Their efforts were numerous and varied. There were parent-child toy libraries (e.g., Nimnicht, Arango, & Adcock, 1977), child-care demonstrations in clinic waiting rooms (e.g., Morris, London, & Glick, 1976), home-visiting programs (e.g., Levenstein, 1970, 1977), and, most comprehensive, the Parent-Child Development Centers (Andrews, et al., 1982), which provided health care, general education, and specific training in child care for low-income mothers. Unfortunately for developmental psychologists, however, the purpose of these programs was not to find out about parents' effects on children's development by changing parents' behavior and assessing what happened. The purpose of the programs was just to change parents' behavior. Most programs, even those receiving federal funds, were not systematically evaluated, and those that were evaluated received the simplest possible assessment: pre and post tests of children's I.Q. Control groups were an anathema to the high-minded principles of social reformers, and quasi-experimental comparison groups were usually not matched with the experimetnal groups except on a few superficial variables. Although quasi-experimental designs may often have been necessary in these studies, it is worth noting that one home-visiting program, which appeared to be very successful when it was evaluated using a quasi-experimental design with a matched comparison group (Levenstein, 1970; 1977), did not do so well in a replication using a randomly assigned control group (Madden, Levenstein, & Levenstein, 1976).

Even when true control groups were used, moreover, researchers encountered practical problems that made evaluation of program effects difficult. These problems included diffusion of the program, as neighbors talked over the back fence about what their home visitor had said or done (e.g., Gray & Klaus, 1970), rising test scores of control group children as a result of repeated testing (e.g., Andrews, Blumenthal, Bache, & Wiener, 1975), and selective attrition of subjects, which was higher in control groups or which interacted in more complex ways with subject characteristics (e.g., Gray and Klaus, 1970).

The major problem with these parent-training studies, though, was that researchers did not evaluate the processes of change occurring in their programs. They did not evaluate parents' and children's behavior before, during, and after the program. Consequently, despite a consistent finding that children's I.Q. scores rose over the course of these programs, researchers and program promoters could not say how or why the gains occurred. They simply based their conclusions on a number of untested assumptions:

-- They assumed that what they intended in designing the program was what actually happened in the field, and they did not monitor program implementation. In a parent-training study that we did in New Haven (Kessen et al., 1975), we had the opportunity near the end of our home-visting program to videotape home visitors <u>in situ</u>. We were amazed to discover how our curriculum guidelines were distorted in the field by even our most willing and well-intentioned home visitors.

-- They assumed that the message the home visitor intended was what got through to the mothers. In our New Haven study we discovered that the messages mothers were picking up from the home visitors were often

oversimplications of what the home visitors intended. For example, the mothers apparently heard "Play with your baby," but they did not pick up more subtle suggestions about how to play with the baby.

-- They assumed that all mothers were ready for parent training. Differences in mothers' attitudes prior to the program have been observed to be related to the mothers' involvement in the program, their behavior, and the child's progress (e.g., Martin, 1977; Schaefer & Aaronson, 1977, and, more recently, Belsky, 1986).

-- They assumed that the mothers' goals for their children were the same as the program designer's, or would be if the mothers knew better. In fact, the program designer's goal might be to foster children's creativity and expressiveness, whereas the low-income mother's goal might be to encourage children's obedience and good behavior. Compatibility between parents' and programs' goals has been found to affect outcomes (Sonquist, 1975).

-- They assumed that increasing mothers' knowledge changed their behavior. Although significant correlations between mothers' knowledge and behavior have been found in observational studies (e.g., Clarke-Stewart, 1973), this does not mean that knowledge causes behavior or that more knowledge causes changes in behavior.

-- They assumed that mothers in parent-training programs changed in the desired direction. In our New Haven study we discovered that mothers' behavior did not always change in the desired direction: many mothers became more directive with their children rather than less, and their sensitivity to their children's behavior was not significantly affected by our efforts.

-- They assumed that the change in mothers' behavior caused the gains in children's performance. In only four studies (Andrews et al., 1975; Forrester, Boismier, & Gray, 1977; Kessen et al., 1975; Kogan & Gordon, 1975) had researchers assessed changes in mothers' behavior and children's development over the duration of their programs. The researchers in these studies had found that changes in mothers' behavior neither consistently preceded nor were significantly correlated with changes in children's behavior. In addition, a number of other studies showed that child-focused intervention programs were at least as effective as mother-focused ones, if not more so, in producing gains in children's I.Q.'s (e.g., Gilmer, Miller, & Gray, 1970; Kessen et al., 1975; Miller & Dyer, 1975) and that the behavior of children in these child-focused programs influenced their mothers' behavior (Falender & Heber, 1975).

Altogether then the parent education movement was based upon untested and probably untrue assumptions. Clearly this most powerful tool for assessing parents' effects on children's development had not been fully exploited.

On the basis of all the studies I had reviewed, I characterized the research on parents' effects on children's development as being single celled--as researchers focused on single cells in the research matrix and on single variables at a time and plied their craft alone in their solitary, monastic academic cells, constrained by their communist cells (or prison cells) to follow single paradigms. I called for replacement with research that was multivariate, programmatic, integrative, collaborative, contextual, process-oriented, and concerned with social as well as merely statistical significance, and I stressed the potential usefulness of the parent-training paradigm for finding out about parents' effects on children's development.

Now, a decade later, it seems appropriate to ask: What progress have we made in finding out about parents' effects on children's development? The answer is complicated. On the positive side, it seems clear that research isn't single celled anymore. There has been considerable integration across areas and across methods, and some attempt has been made to resolve differences among researchers and discrepancies among results. Collaboration between researchers, including those from different cultures, is increasingly common. Research is more likely to be programmatic and contextual and it is very multivariate. Today we do not have such naive assumptions about cause and effect as we did ten years ago, and we are much more careful about confounding variables in our research design. We would never confound SES with other variables, for instance. In fact, we usually don't study SES at all. Now we spend countless thousands of dollars and hours to assess child-rearing environments directly--only to find that SES is strongly related to all our molecular measures and a better predictor of child development than most of them (e.g., Kaye, 1982). More awareness of the limits of statistical significance has appeared; researchers now are more likely to report and discuss the percent of variance accounted for rather than just setting their p level as the ultimate criterion and tallying up their score. Researchers have also become much more concerned with investigating processes of development, rather than just demonstrating group differences. Of course there are still studies dotting our journals that are simply replications of 1970s studies with different groups--adolescent mothers, poor fathers, preterm infants, and so on. But it does seem that there have been substantial and significant changes in the way research is done.

There have also been marked shifts in the content and emphasis of research, away from the parent as influencer to the parent as influenced.

There has been a shift toward emphasizing children as contributors to their own development and, at the same time, a shift toward emphasizing parents as cogs in family networks and pawns of the larger society (Belsky, 1984; Bronfenbrenner, 1979; Radke-Yarrow & Zahn-Waxler, 1986). The issue of the importance of early versus later experience has become a nonissue, laid to rest by the natural attenuation of the two extreme positions.

So now with these improved research methods and these shifts in focus, do we know any more than we did a decade ago about parents' effects on children's development? A poem written about the field of mathematics and quoted by Kagan (1984, p. 275) presages the answer:

> Little by little, we subtract
> Faith and fallacy from fact,
> The illusory from the true,
> And starve on the residue.

We may not be starving yet, but simple generalizations about parents' effects on children's development do seem to be dwindling, and perhaps our confidence with them.

To demonstrate this point, rather than attempting a comprehensive review of all the research on parents and children done over the last 10 years, I have selected five currently active areas of research in which to examine what we have found out and what we still know about parents' effects on development. There are many other areas that could be discussed, but these five will serve to make the point that we still do not have all the answers about whether, how, and how much parents influence their children's development, and that, to some extent, the more we find out the less we know.

The first area of active research is <u>infant temperament</u>. For our purpose here, the issue is not whether temperament exists. It seems that there is

enough evidence of individual differences among infants--in activity, inhibition, cuddliness, irritability, and so on (Buss & Plomin, 1984; Campos, Barrett, Lamb, Goldsmith, & Stenberg, 1983; Thomas & Chess, 1981)--to make temperament a useful construct, even if there are major problems of measurement to be worked out (Crockenberg & Acredolo, 1983). Nor is the issue whether infant temperament influences parents' behavior. I am sure that it must, although the proof is not yet clear-cut (see Crockenberg, 1983)--probably because the effect depends on the characteristics of individual parents as well as the characteristics of individual infants. The question for us here is whether parents' behavior influences infants' temperaments.

To speak to this question, there is, first, research on the stability of temperament over time. As best we can tell with our imperfect measures, for the majority of children, there is substantial stability in temperament from infancy through childhood: easy infants tend to stay easy and difficult children, at least boys, remain difficult (Bates & Bayles, 1984; Guerin & Gottfried, 1986; Korn, 1984; Riese, 1986; Worobey, 1984). For most contemporary researchers who have studied temperament, wishing to prove that it is inborn, this is all that matters. But what about those children who <u>do</u> shift from one temperamental category to another? This shift may depend upon parents' behavior. Despite the fact that infant temperament is an active area of research, this hypothesis has not been confirmed. There are only the slightest hints that it may be true. For example, Washington, Minde, and Goldberg (1986) report that the mothers of infants who were easy at 6 months and difficult at 12 months were highly insensitive toward the infants at 6 months, whereas the mothers of infants who were difficult at 6 months and easy at 12 months were highly sensitive at 6 months. Some research does not even provide hints. For example, based on the New York Longitudinal Study data,

Lerner and Galambos (1985) claim that mothers' rejection causes children to develop difficult temperaments. They make this claim because the path coefficient between mothers' rejection of their children at age 3 and the children's likelihood of having difficult temperaments at age 4 was .25, whereas the path coefficient between children's difficult temperaments at age 2 and at age 4 was only .17. But the difference between these two path coefficients is small and could be the result of the difference in the length of time between assessments. (Higher cross-time correlations are always obtained when temperament measures are assessed at shorter time intervals.)

According to one theory (Buss & Plomin, 1984) parents are more likely to influence infants in the middle range of temperament, whereas at the extremes infants influence their parents. Again, this hypothesis has not been confirmed, but we have a hint in recent research. Crockenberg and McCluskey (1986) found that differences in parental caregiving had a greater influence on easy babies than on difficult ones; specifically, responsiveness to infants' crying was correlated with less frequent crying later for infants who cried infrequently as newborns but not for infants who as newborns were frequent criers.

According to another theory, parents do not have simple effects on infant temperament, but the goodness of fit or mesh between their behavior and expectations and the infant's temperament influences the infant's development (Lerner, 1984; Thomas & Chess, 1977). For example, an easy baby might do well with a mother who lacked self-confidence but poorly with a mother who wanted an assertive baby, whereas a difficult baby might do well with a mother who wanted an assertive child but poorly with a mother who lacked self-confidence. Judging by the debate in a conversation hour at the recent International Conference on Infant Studies (Crockenberg, Bates, Plomin,

Sroufe, & Wachs, 1986), the issue of the interaction between infants' temperaments and parents' behavior is far from settled. As even proponents of the goodness of fit theory admit (e.g., Windle & Lerner, 1986), regarding this question, we are stronger on theory than on substance. In fact, no adequate tests have yet been done of the goodness of fit hypothesis. What is needed for such tests is research using objective measures of infants' temperaments and independent measures of parents' behavior, collected longitudinally on large samples, following specific hypothesized parent-child links rather than the vague, general notion of goodness of fit.

A second area of great activity and debate in recent years has been infant attachment. There are now dozens of studies and discussions about "A babies," "B babies," and "C babies," and what these classifications mean. Our purpose is not to argue the virtues of A's, B's, or C's. Our question is what is the effect of parents on the development of infants' attachments. Since an attachment, by definition, is a relationship, it seems obvious that the parent to whom the attachment is formed would contribute to the development of the bond. Nevertheless, some developmental psychologists have claimed that attachment is just temperament in a new guise (Chess & Thomas, 1982; Kagan, 1982). The problem may have arisen because of researchers' reliance on a single procedure for assessing infants' attachment: the strange situation. Temperament clearly does influence infants' behavior in this situation. For example, the likelihood that an infant will show distress is related to his or her overall temperamental irritability. But the classification of children's behavior in the strange situation into A, B, and C categories is supposed to rise above infants' specific behaviors, like crying, to capture the organization of infants' behavior into psychologically meaningful patterns (Sroufe & Waters, 1977). Infants who cry little could fall into attachment

category A (insecure avoidant attachment) or into category B1 (secure attachment); infants who cry a lot could be found in category C (insecure ambivalent resistant-attachment ) or in category B4 (secure attachment). Evidence that attachment is not simply temperament comes from research showing that an infant's attachment classification changes if there are changes in the infant's living circumstances (Vaughn, Egeland, Waters, & Sroufe, 1979) and from research showing that infants often form qualitatively different attachments to their mothers and fathers (Grossman, Grossman, Huber, & Warner, 1981; Lamb, Hwang, Frodi, & Frodi, 1982; Main & Weston, 1981). Unfortunately, there are problems with these studies because it is not clear whether the strange situation was equally strange for infants at the two assessments or if the time lag between assessments increased the likelihood of getting different attachment classifications. Nevertheless, for the time being, it seems reasonable to assume that attachment is not just temperament.

Even if they are not the same thing, though, there are many ways in which temperament and attachment could be related. Temperament might influence the parents' behavior, which would then influence the child's attachment; temperament and attachment might be related to the same kinds of parental behavior; or temperament might affect the attachment subgroup classification (A versus C or B1 versus B4) whereas parents' behavior would influence the overall security of attachment (B versus A or C). In fact, researchers in a number of studies have found an association between temperament and attachment security (e.g., Holmes, Ruble, Kowalski, & Lauesen, 1984; Maslin & Bates, 1983). But results are inconsistent. Not all researchers have found such a link (e.g., Belsky, Rovine, & Taylor, 1984; Egeland & Farber, 1984; Singer, Brodzinsky, Ramsay, Steir, & Waters, 1985; Weber, Levitt, & Clark, 1986). More consistent findings relate characteristics of infants to attachment

subgroups: insecure avoidant (A) babies are likely to be easy, robust, interested, and persistent; insecure ambivalent-resistant (C) babies are likely to include those who are chronically ill or difficult, and who, as newborns, had low Apgar scores, low Brazelton scores, and low birth weights (Connell, 1976; Waters, Vaughn, & Egeland, 1980; see Goldsmith, Bradshaw, & Rieser-Danner, 1986). Clearly, more research is needed to establish the contribution of temperament to attachment--and vice versa.

More important for us here, though, is research in which investigators have looked for the link between attachment classification and parents' behavior. A decade ago, with only a couple of studies completed (Ainsworth, Blehar, Waters, & Wall, 1978; Clarke-Stewart, 1973), the link between attachment security and maternal sensitivity seemed secure. In fact, it is ironic that, as Sroufe (1985) points out, the strange situation was introduced and widely used <u>because</u> it reflected patterns of infant behavior that were related to maternal sensitivity. Now, there are those who dispute the connection (e.g., Lamb, Thompson, Gardner, & Charnov, 1985), because in the numerous studies over the last 10 years the link has not always appeared. My own view is that if we demand total consistency across different studies and different measures, we are doomed to frustration. (That's why we have theory and metaanalysis to test and make sense of the consistency we can find.)

So what consistency have recent studies revealed about the link between parents' behavior and children's attachments? There are three studies in addition to Ainsworth's original one that have documented an association between ratings of mothers' sensitivity at 6 months and infants' attachment security at 12 months (Bell, in preparation, Egeland & Farber, 1984; Grossman, Grossman, Spangler Suess, & Unzer, 1985). Other studies offer support for this association by documenting links between attachment security and the

following kinds of maternal behavior: appropriate responsiveness (Smith & Pederson, 1983), prompt responsiveness to distress (Crockenberg, 1981), warmth, involvement and responsivity on the HOME Scales (Bates, Maslin, & Frankel, 1985), ratings of observed warmth, responsiveness, and happiness (Gaensbauer, Harmon, Culp, Schultz, van Doorninck, & Dawson, 1985), sensitivity and acceptance inferred from interviews (Benn, 1986) and observed moderate, appropriate stimulation and responsiveness (Belsky, Rovine, & Taylor, 1984). Behavioral measures of maternal care, such as the quality of physical contact or the mother's caregiving facility, are not always correlated with children's attachment security (e.g., Egeland & Farber, 1984), but the problem is probably with the measures not with the hypothesis about maternal sensitivity.

In addition to the link between maternal sensitivity and children's overall secure/insecure attachment classification, links have been found between maternal characteristics and children's attachment subgroup classifications. Among secure infants, B2 and B3 babies are most likely to have the most sensitive mothers (Goldberg, Perotta, Minde, & Corter, 1986). Among insecure infants, A babies are most likely to have mothers who are insensitive, unresponsive, angry, tense, intense, intrusive, and interfering, and in the extreme, physically abusive; C babies' mothers are most likely to be insensitive, inept, unsupportive, unadaptable, and in the extreme, neglectful (Crittenden, 1983; Egeland & Sroufe, 1981; George & Main, 1979; Radke-Yarrow, Cummings, Kuczyncki, & Chapman, 1985; Schneider-Rosen & Cicchetti, 1984; Weber, Levitt, & Clark, 1986). There does seem to be a significant degree of predictability from parents' behavior to infants' attachment classification.

Even better predictability of infants' attachment classification is possible if we include in the analysis not only mothers' behavior and infants' characteristics but social conditions as well. Belsky and Isabella (in press), for example, found that when mothers were sensitive, infants easy, and mothers and fathers happily married, 11 out of 12 infants (92%) were securely attached to their mothers; when mothers were insensitive, infants difficult, and mothers and fathers unhappily married, 5 out of 6 infants (83%) were insecurely attached to their mothers. Crockenberg (1981) also found that a combination of factors had a high predictability of attachment security: difficult infants with insensitive mothers who experienced low social support were likely to be insecurely attached. Similarly, Radke-Yarrow et al. (1985) found that infants of mothers who were depressed and who had no husband living in the home were likely to be insecurely attached. And in research by Barglow, Vaughn, and Molitor (in press) it was found that infants who were in poor day care and who had single, poor mothers plus the added vulnerability of being boys were most likely to be insecurely attached.

It does seem that within the constraints imposed by temperamental and physiological characteristics of the infant and social and economic conditions in the family, parents' behavior does make a difference in the quality of infants' attachments. But there are still open questions about the effects of parents' behavior on the development of infants' attachments. For example, if an infant starts out with a difficult temperament, what kind of sensitivity is required of a parent to produce a secure attachment in that infant, and what is the likelihood? If a baby has an easy temperament, how insensitive must the parent be to produce an insecure attachment? The clearest results demonstrating a link between parents' behavior and infants' attachment have been obtained in research in which parents are so extremely insensitive as to

be abusive. But how marked are the effects of parents' sensitivity within the normal range of parents? Another open question in this area is whether there are different routes to the same attachment category, or at least to the same pattern of behavior in the strange situation. Can avoidance of the mother in the strange situation, for example, be the result of deliberate independence training or of familiarity with separations from the mother, as well as the result of mothers' insensitivity.

The kind of research needed to answer these questions requires, first of all, validating assessments of children's attachment [for example, Waters & Dean's (1985) Q sort of behavioral terms, or extension and modification of the strange situation], and independent assessments of parents' sensitivity (for example, experimental investigation of mothers' behavior with several different infants). Then, researchers would be advised to study the development of attachments longitudinally, focusing on mothers who change in sensitivity and infants who change attachment categories. Egeland and Sroufe's (1981) prospective study, which demonstrated the increased likelihood of avoidance from 6 to 18 months for infants whose mothers even at the infants' births were emotionally detached and uninvolved is an example. Changes in infants' attachment classifications should be viewed not as a nuisance that spoils the demonstration that attachment classifications are stable, but as a natural experiment--an experiment that could inform us about the conditions necessary for mothers to act sensitively and for infants to develop securely, and about the effects of mothers on infants and vice versa. Researchers might also investigate how the sensitivity of caregivers other than mother, such as fathers and babysitters, is linked to the development of children's attachments to these caregivers (e.g., Easterbrooks & Goldberg, 1984). And finally, researchers might pursue the parent-training

paradigm, designing programs to enhance parents' sensitivity to infants' needs and expressions and carefully monitoring the effects of their programs on both parents and infants. Efforts to improve the quality of mothers' caregiving have consisted of clinical therapy for disturbed mothers or brief interventions with high-risk mothers such as having mothers watch their infants being tested on Brazelton's (1973) Neonatal Behavioral Assessment Scale (e.g., Widmayer & Field, 1980; see Worobey & Brazelton, 1986). The assessments and outcomes of these interventions have been as modest as the interventions themselves. Effects on infants' attachment development have not been assessed, and effects on mothers' caregiving have not been replicated in middle-class samples (Belsky, 1985). A more substantial and successful effort to increase the caregiving sensitivity and responsivity of mothers of preterm infants was made by Barrera, Rosenbaum, and Cunni gham (1986), but again effects on infants' attachment were not assessed. The exploration of parent effects on attachment using training interventions is far from fully realized.

A third area of active research in the 1980s is the effect of parents on children's cognitive development. In this area several new trends have appeared in the last decade. I will mention three of them. The first is a trend toward greater specificity--of cognitive outcomes and parents' inputs. Rather than looking for links between overall levels of cognitive development and overall levels of stimulation as researchers did in the 1970s, researchers now have begun to probe ties between particular kinds of parental behavior and children's knowledge and skills in particular domains: academic knowledge (Price, 1984; Price, Hess, & Dickson, 1981), memory (Mills & Funnell, 1983; Ratner, 1984), reasoning (Donaldson, 1983) and problem solving (Kontos, 1983; Saxe, Gearhart, & Guberman, 1984), for example. They have done this by looking at actual teaching situations and observing links between what the

parent does and what the child does. Analyses are for the most part descriptive (i.e., analyses of transcripts) rather than correlational, fleshing out the notions of scaffolding (Wood, 1980) and the zone of proximal development (Rogoff & Wersch, 1984).

These studies give compelling evidence that parents can act as instructors for their children and that in interactions with them children perform at higher levels than by themselves. In an exemplary study, Wood (1980) observed mothers teaching both their own children and unfamiliar children in a construction task. Mothers' performance with the two children was correlated and so was the two children's performance. This suggested that mothers were having an effect on the children's performance. To confirm this, Wood then implemented an experimental intervention in which an adult taught children the construction task using the four instructional strategies that the mothers had spontaneously exhibited. The strategy of contingent instruction that had been most successful in the mother-child interactions turned out to be most effective in the experimental intervention--supporting the suggestion that mothers influence children's performance. Whether it makes a difference in the long run how and how much parents act as instructional guides for their children is not clear from these studies. These studies do not speak to issues of development, but rather performance. Even in the short run, moreover, parents' help may not be necessary to enhance children's performance. Kontos (1983), unlike many researchers studying the zone of proximal development, had the foresight to assess the problem solving progress of a control group who did not have their parents' help; she found that the performance of these children improved too, solely as a function of practice.

A second trend observed over the last decade in the study of parents' effects on children's cognitive development--the opposite of this trend toward greater specificity--is a trend toward greater breadth. This is evident in the increased use of multivariate designs and analyses. In their efforts to see the "big picture," researchers using multivariate data collection (such as the HOME scales or multiple measures of their own devising) have accounted for a substantial percent (30% to 60%) of the variance in children's I.Q. scores (e.g., Carew, 1980; Gottfried, 1984; Olson, Bates, & Bayles, 1984; Stevens & Bakeman, 1985). They also have attempted to identify the best predictors of children's intelligence. The best predictors they have found so far, are the variety of play materials and experiences provided the child and the mother's efforts to support advances in the child's development. To find these predictors, researchers have used multivariate analyses like partial correlations, regression analyses, path analyses, and structural equations (Lisrel). The problem with fancy statistics like structural equations though, is that they only work with big samples--samples that are seldom available to researchers in this area. Unfortunately, some researchers seem to have gotten carried away by their fancy statistical models and forgotten that the analyses are only as strong as the data, the model is only useful if the results make sense, path analysis is no substitute for a longitudinal design, multivariate analyses do not establish causal direction, and the simplest analysis is still sometimes the best. Increased use of multivariate analyses does not yet appear to have made a forest out of the trees connecting parents' behavior and children's development.

Multivariate analyses have been helpful in one important way, though. They have demonstrated that children's intelligence levels cannot be predicted from parents' behavior--even multivariate indexes of same--alone.

Intelligence is also related to child factors. To give one example, researchers have shown that children's intelligence is related to both the organization of sleep states in infancy and the mother's stimulating behavior (Beckwith & Parmelee, 1986). In a similar example, researchers have shown that children's intelligence is related to both speed of habituation and the mother's encouragement to look at new objects in infancy (Bornstein, 1986). In another example, researchers have shown that what is moderate and intelligence-enhancing stimulation for temperamentally easy infants can be overwhelming and unproductive for difficult infants (Wachs & Gandour, 1983). Multivariate analyses allow researchers to include both child factors and parent factors in their analyses. Multivariate analyses also allow researchers to include both genetic factors and environmental factors in their analyses. Using partial correlations, researchers have shown that mothers' I.Q. levels (with HOME environment partialled out) predict children's I.Q. levels at 2 years and that HOME environments (with mothers' I.Q. partialled out) predict children's I.Q. levels at 3 to 4 years (Wilson, 1983; Yeates, MacPhee, Campbell, & Ramey, 1983). This kind of analysis suggests that parents' behavior become more important as children actively begin to explore the world. But it does not establish the extent of parents' genetic contribution to their children's intelligence. For that we must turn to behavior genetics studies--the third new trend in this area of parent effects on children's cognitive development.

It has always been clear to behavior geneticists, if not to developmental psychologists, that the contributions of heredity and environment to development cannot be separated in natural, single-child families. Unfortunately, however, until the last decade behavior geneticists and developmental psychologists did not mix much. Developmental psychologists did

not consider genetic contributions to their parent-child correlations, and geneticists did not assess the effects of the environment on their concordances. Now, thanks to a few researchers who are comfortable in both areas, there is more mixing. Behavior geneticists are using HOME scales and developmental psychologists are demanding twins and zygosity tests. Actually, because the concordance between the I.Q. scores of twins reared together reflects the contributions of both similar environments and identical genes, researchers should be demanding twins reared apart or, easier to find, the children of twins. In one study of the children of twins (Rose, 1979), for example, the researcher found that there was no significant difference between the correlations of children with their own parents and with their aunts or uncles who were identical twins of their parents, on a nonverbal I.Q. measure. The study thus demonstrated the strong contribution of genetics to one aspect of intelligence and presented a useful design for future research.

Even easier to find than children of twins are adopted children. Comparing the correlations between adopted children's scores and their adoptive parents' and biological parents' scores is another way researchers can learn about genetic contributions to intelligence. Researchers using this design, such as Scarr and Weinberg (1981, 1983) in Minnesota, Plomin and DeFries (1983) in Colorado, and Horn (1983; Horn, Loehlin, & Willerman, 1982) in Texas, have found strong evidence for genetic effects. They have found, for instance, that correlations between biological mothers' I.Q. scores and their natural adopted-away children's I.Q. scores are as high as correlations between living-together biological mothers and their children. They have also found that the correlation between adopted children's I.Q.'s and their biological parents' I.Q.'s is higher than the correlation between adopted children's I.Q.'s and their adoptive parents' I.Q.'s (e.g., .30 versus .15 in the Texas Adoption Project).

Yet another indication of a genetic contribution to I.Q. that shows up in adoption studies is what Plomin, Loehlin and DeFries (1985) call "genetic mediation" of parents' effects on children's development. In the Colorado adoption study they found that 28 out of 34 correlations between parent behavior measures and child development measures were higher in biological families than in adoptive families--many of them significantly so. The mean correlation coefficient between HOME scales and child development measures was .24 in biological families; in adoptive families it was only .09. Interestingly, partialling out parents' I.Q. levels did not reduce the correlations obtained in biological families, so "genetic mediation" does not occur via parental I.Q. (It also is apparently not a statistical artifact, because the means and ranges of parent and child measures were equivalent in adoptive and biological families.) The mediation could be the result of some other genetic factor, as Plomin, Loehlin, and DeFries suggest, or it could be the result of psychological factors such as parents' attitude and beliefs. This idea of genetic--or psychological--mediation is a new one, an idea that adds a new complexity to the study of parent effects on children's development and clearly merits further investigation and thought.

But adoption designs not only demonstrate the heritability of intelligence, they also provide evidence for the effects of parents' behavior on children's development. This evidence comes from examining the mean differences in I.Q. between adopted and nonadopted children. In the Texas adoption study, for example, Horn found that the mean I.Q. of the adopted children was the same as the mean I.Q. of the adoptive parents (112), whereas the mean I.Q. of the biological parents from whom the adopted children had been taken was significantly lower (108). In other studies, researchers have found that the mean I.Q. of black children adopted by white parents is higher

than the mean I.Q. of black children who remain in the ghetto (Scarr & Weinberg, 1981) or who are adopted by black parents (Moore, 1986). Clearly, the home environment does matter. In fact, in Plomin, Loehlin and DeFries's study there were two HOME factors that predicted children's I.Q. and showed no "genetic mediation": variety of toys and experiences, and maternal involvement with the child. In adoptive families as well as biological ones these factors were significantly correlated with children's I.Q. scores.

Evidence for the effect of parents' behavior on young children's I.Q. scores might also be inferred from research on siblings--another behavior genetics design that is growing in popularity. Studies show that correlations between the I.Q. scores of young siblings are high and that mothers treat young siblings alike, especially in ways that might enhance their intelligence (e.g., verbal responsiveness), whether the children are genetically related or not (Dunn, Plomin & Daniels, 1986; Dunn, Plomin, & Nettles, 1985; Scarr & Weinberg, 1976). Studies of siblings also show that later in the school years and in adolescence, as the environments of siblings become more different, siblings' I.Q. scores diverge and are no longer correlated (Scarr & Kidd, 1983).

On the basis of all these studies done over the past decade, we can infer that parents' behavior does affect children's intelligence--within the constraints of genetic variation. What we still have not pinned down are the limits of the effect. As I suggested a decade ago, we could learn something about this issue from experimental intervention, that is, from parent training studies. Apart from Glen Doman's (1984) unevaluated efforts to create super babies, recent intervention studies focused on cognitive development have been disappointingly few in number and modest in scale. There was Belsky, Goode, and Most's (1980) subtle home intervention in which mothers' attention was

drawn to their own behavior every time they focused their infant's attention on an object (an intervention that led to increased maternal focusing and increased infant competence). There was Barrera, Rosenbaum, and Cunningham's (1986) more substantial intervention with mothers of preterm infants, which increased the families' HOME scores and the infants' Bayley mental scale scores compared to untreated controls. But that is about all. Slaughter's (1983) study demonstrating the effectiveness of discussion groups for increasing low-income mothers' ego development, teaching styles, and interaction with their children and their children's I.Q. scores, was in fact carried out in the 1970s. Perhaps this lack of research on parent training reflects the shift over the past decade in the availability of funding. Perhaps it reflects our decreased optimism concerning social change. Perhaps it reflects the shift in emphasis within developmental psychology from a focus on external conditions to a focus on internal constraints.

Nowhere is this emphasis on internal constraints clearer than in the next currently active area of research--language development. While it is obvious that the environment must contribute to children's language development--English children learn English, French children learn French--the questions of how much the environment contributes, what about the environment contributes, and what the environment contributes to, are open. Inspired by Chomsky (1965), the goal of most language researchers has been to demonstrate the existence of innate structures in language or innate language abilities in children--abilities like infants' capacity to perceive phonemic categories (Eimas, Siqueland, Jusczyk, & Vigorito, 1971), deaf children's ability to develop spontaneous systems of language-like gestures (Goldin-Meadow & Mylander, 1985), and hearing children's abilities to map single concepts onto single words (Gleitman & Wanner, 1982; Mervis, 1985; Slobin, 1973), to make

hypotheses and generalizations about syntax (Gleitman & Wanner, 1982) and to expect nouns (like "dog") to refer to basic level object categories (like dogs) rather than superordinate categories (like animals) or functional associations (like dog and bone) (Horton & Markman, 1980; Markman & Hutchinson, 1984). Only a few researchers have asked what and how the environment contributes to children's language development.

In this area, it is important to make the distinction between how the environment contributes to language development in all children and how it contributes to individual differences in language development between children. (The same point could have been made about the effects of the environment on the development of children's intelligence.) Just because something does not contribute to individual differences between children does not mean that it is not important, or even essential, for development. It just may be that, like vitamin C, all parents provide it and all children get it, at least in sufficient amounts to promote their development (cf. Bates, Bretherton, Beeghly-Smith, & McNew, 1982).

With respect to the contribution of the environment to language development in all children, relevant research shows that the words mothers speak to their young children are used by the children; for example, mothers and children both use basic level category words like "dog" rather than superordinate or subordinate words like "animal" or "Collie" (Anglin, 1977; Blewitt, 1983; Mervis & Mervis, 1982; Rosch, Mervis, Gray, Johnson, & Boyes-Braem, 1976). It shows that morphemes that are more frequent and acoustically distinct in the mother's language to the child appear earlier in the child's speech (Moerk, 1983). It also shows that young children are tuned into the qualities of "motherese"--the special speech register adults use when talking to infants and toddlers, consisting of short simple sentences, frequent

repetition, high pitch and exaggerated pitch contours, and emphasis on the here and now--and these qualities of motherese probably make it easier for all young children to understand and learn language (Berko-Gleason & Weintraub, 1978; Fernald, 1984). Short sentences are helpful because young children have short memories. Simple sentences help children pick out important words. Pauses help children separate syntactic units (Hirsh-Pasek, Nelson, Jusczyk, & Wright, 1986). High pitch makes language salient and attractive (Fernald, 1985; Sullivan & Horowitz, 1983). Emphasis on the here and now teaches the names of the objects or events to which the child is attending (Masur, 1982). Repetition of utterances in slightly different forms illustrates different syntactic arrangements and is related to children's use of longer and more complex sentences (Hoff-Ginsburg, 1986).

It once was thought that motherese was the key to children's language development, a way mothers had of teaching their children language. It now seems clear that motherese is a response to children's language abilities, rather than the cause (Bohannon & Hirsh-Pasek, 1984; Gleitman, Newport, & Gleitman, 1984; Kaye & Charney, 1981; Shatz & Gelman, 1977). In order to make their message clear, people talk motherese to children, to dogs (Hirsh-Pasek & Trieman, 1982), and to foreign (Bohannon & Warren-Leubecker, 1982) and retarded adults (Pratt, Bumstead, & Raines, 1976). Motherese is not a language teaching strategy. Moreover, infants learn language even without a steady diet of motherese, for example in the Pacific Kaluli (Schieffelin & Ochs, 1983) and the Quiche Mayan cultures (Ratner & Pye, 1984). The current view seems to be that parents' speech provides children with data about language which illustrate language regularities and feed into children's proclivities to look for regularities and to use them as the basis for generalizations about words and sentences. Parents' speech also prods

children to use and create language by asking them questions and engaging them in conversations.

But does parents' language--as either input or prod--contribute to individual differences in the rate at which children learn language? This is difficult to determine. Parents obviously speak to the child long before the child begins to talk. Therefore, one cannot simply use temporal order as an index of causal direction. Researchers have settled for using gains in children's language--in size of vocabulary, comprehension, mean length of utterance, noun phrases per utterance, verb phrases per utterance, number of auxiliary verbs, etc.--over some period of time, usually a few months, partialing out the child's initial language level and age. Using this strategy, researchers have found significant correlations between children's language gain and mothers' verbal input (number of utterances, noun phrases per utterance, number and percent of WH questions, other kinds of questions, and directives), verbal prodding (accepting the child's language and asking questions that demand sentences as answers), and simple expansions of children's utterances (Barnes, Gutfreund, Satterly, & Wells, 1983; Cross, 1981; Furrow, Nelson, & Benedict, 1979; Gleitman, Newport, & Gleitman, 1984; Hoff-Ginsburg, 1986; Moerk, 1976; Nelson, 1980; Nelson, 1973; Olson, Bayles, & Bates, 1986). Researchers have not found that children's language gain is consistently correlated with motherese. While some degree and amount of motherese may help children acquire language faster, more motherese is not necessarily better. There is no simple correlation between parents' syntax and the child's (Chesnick, Menyuk, Liebergott, Ferrier, & Strand, 1983; Kavanaugh & Jirkovsky, 1982; Nelson, Denninger, Bonvillian, Kaplan, & Baker, 1983; Newport, Gleitman, & Gleitman, 1977). In fact, when the mother's mean length of utterance is reduced to the child's level, it is negatively related

to the child's language (Clarke-Stewart, VanderStoep, & Killian, 1979; Gleitman, Newport, & Gleitman, 1984).

Of course, heredity also contributes to individual differences in language development, just as it does to the development of intelligence. This has been demonstrated in behavior genetics studies. For example, in the Colorado Study (Hardy-Brown, Plomin, & DeFries, 1981), the correlation between the child's language and the biological mother's I.Q. was greater than the correlation between the child's language and the amount the adoptive mother's talked or read to the child. Similarly, in a study of identical and fraternal twins (Mather & Black, 1984), children's vocabulary comprehension was found to be more concordant in identical twins than fraternal, which indicates a strong genetic component in this aspect of language development. Nevertheless, the research suggests that there are as well environmental effects on language development.

To demonstrate the effects of the environment on children's language development, a few researchers have used natural experiments. For example, Tomasello, Mannle, and Kruger (1986) studied the language environment of twins, who typically have delayed language development. They found that these children received significantly less verbal input than non-twins from the mother, who had to divide her attention between them. To test the limits of the effects of parents' speech on their children's language development, however, researchers must use true experiments. This has been done infrequently. Perhaps the clearest demonstration of this research strategy is provided by Nelson (1981). He had mothers or an experimenter recast, or expand, children's verbal utterances from the time the children were 22 months old until they were 27 months old. Children who heard simple recasts of their sentences gained most in the mean lengths of their utterances. Children whose

mothers expanded their verbs gained specifically in verbs. Those who heard recast questions gained specifically in questions. Unfortunately, this research strategy has not become popular.

To extend our knowledge of the effects of parents on their children's language development, what is needed is research on large samples of children and parents (to obtain sufficient variability and stable correlations), research in which observers sample the complete corpus of language input to children extensively and longitudinally. This naturalistic research should then be followed by experimental research to test specific hypotheses about the links between parents' and children's language. What we do not need for learning about language development is microanalysis of mother-infant interaction, in the belief that this contributes to individual differences in children's language (e.g., Kobayaski, Ishii, Watanabe, Takahashi, & Kato, 1984; Lester, Hoffman, & Brazelton, 1985). Microanalysis provides detailed descriptions of parent-child interaction, and may show something about how the partners influence each other's immediate behavior, but it has not yet been successfully linked to individual differences in children's long-term development (cf. Kaye, 1982).

The last area of active research I will mention is socialization; that is, how parents influence their children to be good or bad. There are many ways parents socialize their children. They are models, managers, teachers, conveyors of social norms, and providers of emotional milieux. Most recent research, however, has focused on the socializing influence of parents' disciplinary styles. Consistent with earlier research, recent studies have documented a link between children's positive behavior and a disciplinary style that is authoritative--warm, reasonable, nonpunitive, and firm (e.g., Londerville & Main, 1981; Power & Chapieski, 1986; Stevenson-Hinde, Hinde, &

Simpson, 1986; Zahn-Waxler, Radke-Yarrow, & King, 1979). But in this area, as in those already discussed, the new emphasis over the last few years has been a focus on the child's contribution to the socialization process.

Focusing on the child's contribution to discipline, it has been suggested that authoritative parents are able to act reasonably and firmly because their children are reasonable and compliant, rather than the reverse (Lewis, 1981). Authoritative discipline apparently is not necessary for promoting competence, because girls in harmonious families and boys in nonconforming families (two other disciplinary styles) are as competent as children in authoritative families (Baumrind, 1979). In fact, it has been shown that children act prosocially at an early age--before parents even begin their socialization efforts (Hay & Rheingold, 1983; Radke-Yarrow, & Zahn-Waxler, 1986; Rheingold & Emery, 1986). As further evidence of children's contributions to socialization, it has been demonstrated that children's temperaments affect both their good and bad behavior and their parents' socialization efforts (Bates, 1980; Gordon, 1983; Lee & Bates, 1985; Maccoby, Snow, & Jacklin, 1984; Olweus, 1980; Patterson, 1980). Hot tempered, difficult children, researchers have found, act more aggressive. This leads to more controlling efforts by their mothers, more prohibitions, power assertions, and what Patterson (1982) called "nattering." But these children are more likely then to ignore the mother's requests, because they are less sensitive to social feedback. As a consequence, discipline is less effective. So mothers exert less socialization pressure. This leads boys to become more difficult....a vicious, coercive cycle. Related to these studies showing that children's characteristics are related to parents' socialization efforts and effectiveness are studies showing that children's characteristics predict long-term outcomes of parental discipline. For example, over time,

punishment leads to decreased aggression in children who were already nonaggressive, but to increased aggression in children who were aggressive (Eron, 1982). Abrasive discipline leads to greater social competence in girls, but to lower social competence in boys (Baumrind, 1979).

Another example of research that demonstrates children's contribution to socialization is research showing that controlling efforts made by mothers toward their two children at the same age are not highly correlated (e.g., Bryant & Crockenberg, 1980; Dunn, Plomin, & Daniels, 1986). Yet another example is research showing that children are most likely to comply with their mother's requests when they already are oriented toward the task (Martin, 1981; Schaffer & Crook, 1980). Finally, research demonstrating children's contribution to socialization includes studies following various experimental procedures. These studies have demonstrated that adults' behavior is affected by the type of child they are paired with in a laboratory task. Conduct disordered children or children who have been trained to act assertive, responsive, or shy elicit appropriate responses from adults they have never met before (e.g., Anderson, Lytton, & Romney, 1986; Brunk & Henggeler, 1984; Bugental & Shennum, 1984).

It is clear that simple generalizations about one-sided effects of parents' discipline on children's development are no longer possible. As in the other areas we have discussed, researchers and parents need to take the characteristics of individual children into account in understanding the processes involved in development. The area of socialization, too, could benefit from longitudinal observation combined with experimental intervention. Experimentation focussed on children's immediate compliance with an adult exerpimenter in the laboratory has been fairly common, but what has been lacking is systematic intervention into the socialization process at

home. Intervention was not even mentioned as a method for studying socialization in Maccoby and Martin's (1983) comprehensive review of the socialization research in the Handbook of Child Psychology. An example of the kind of intervention research that is needed is Patterson's work (Patterson, Chamberlain, & Reid, 1982) in which researchers attempt to reduce problem children's aggressiveness by having their parents act more authoritative. Other skill training programs for parents with problem children have also been attempted (reviewed by Levant, 1983), as have behavior modification programs (Graziano, 1983) and programs for abusive parents and other specially targeted groups (Dangel & Polster, 1984). But we need such training programs done with normal parents and children, focused on engendering positive behavior as well as eliminating negative behavior, and we need to know what it is about the training that leads to change. Is it, for example, that parents learn to use new socialization strategies, that they perceive their children in new, more reasonable and realistic ways, or that they themselves benefit from increased structure and support? We need to implement and evaluate parent training programs in this area for the same reasons as we did in the areas of attachment, cognitive, and language development. Moreover, in recent research on socialization there is another justification for parent training programs. Researchers have begun to show that parents' socialization strategies are connected to their beliefs and attributions about child development (Sigel, 1985) and their interpretations of children's behavior (Bugental & Shennum, 1984). This gives further support to the view that what is called for is programs in which parents would be educated about child development in general and about their own children in particular.

As this mini review of currently active areas of research makes clear, the more we have found out about parents' effects on children's development,

the more we have found out there is to learn. We have made progress over the past decade, but the progress has been expansive--problem finding rather than problem solving, divergence rather than convergence. In that, our progress well reflects real life and represents healthy development. We certainly have a better idea now than we did ten years ago about the complexity involved in predicting or promoting parents' effects on children's development. We have tapped into the myriad of factors that influence development, and we are less naive now about how easily we can predict or control children's development. This is all to the good. But we need convergence, too, and to achieve that convergence we need a clearer picture of where we are going and how we can get there.

Where we are going, I submit, can be finding out about the limits of parents' effects on their children's development. Obviously, parents do influence their children and children do influence their parents and their own development. But the question is how much can parents influence children's development. How to get there? Clearly, we need to continue our multivariate, integrative efforts, absorbing and accounting for more and more of the variance in children's development--incorporating family systems and physiological givens as we confront complexity head on. But in addition, one major theme that has arisen in the course of this review is that we need to exploit systematic experimentation. We seem to have forgotten our experimental roots in our move to the field. Naturalistic observations are necessary; correlations are useful. But our strongest suit as developmental psychologists may still be causing and measuring change, systematically and experimentally. If we want to find out about parents' effects on children we need to cause effects that we can assess and replicate. This is especially appropriate because our interest in parents' effects on child development is

not merely academic. We are interested in this issue, at least in part, not just so we can understand, describe, and predict development, but so we can change it. Parent training programs can be both the medium and the message in this area of research.

If we are to understand the processes of parents' influence on their children's development, we also need in our research to enrich our methods and our analyses. Just as we left behind experimental designs, we have left behind clinical methods of assessment and put our faith in numbers rather than insights. Most researchers currently do what might be called "hour-glass research." We start with the complexity of "real life," reduce it to tiny numbers, and infer from them the "big picture." We underutilize qualitative methods like ratings and semi-structured situations for assessment, even though ratings can be more stable, predictive, and psychologically meaningful than a computer-full of molecular measures and microanalyses (e.g., Clarke-Stewart & Hevey, 1981; Bakeman & Brown, 1980). We have replaced intellectual effort with statistical analysis, forgetting that results are only as good as the variables that go into them and the interpretation that comes after them. We need numbers--large numbers--but we need to combine the numbers with understanding. To aid in our understanding, we also need to enlarge our repertoire of statistical methods. We seldom look for curvilinear relations or the effect of single critical events (although they clearly exist, e.g., Roberts, 1986; Roe & Roe, 1984). We need to enrich our assessment and analysis tools, thoughtfully interpret our analyses, and follow the research designs--both correlational and experimental--that will be most informative, as we do the hard work of bringing together in the next decade of research all that we have learned in the past one. Only then can we look forward to a decade of real progress.

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